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is subject to the action of the next International Congress, to be held in Great Britain in 1898.

A large edition of the International Code has been issued by the Société Zoologique de France for general distribution, and any working zoölogist may obtain a copy of it by addressing the Secretary, Prof. Raphael Blanchard, 32 rue du Luxembourg, Paris, France.

Should any American zoölogist have any suggestions to make in regard to additions, amendments, etc., to this Code I request him to communicate with me before September 1, 1896.

There are a few points in this Code with which perhaps the majority of American zoölogists are not in sympathy. 'The majority of American zoölogists,' however, does not mean 'the majority of the zoölogists of the world,' and while it is beyond question that a view expressed by the majority of workers in this country will receive the utmost consideration, we must not forget that the next International Congress cannot be expected to repeal the present International Code and adopt an American, French, German or English Code in its place, but must stand by international decision upon all questions in regard to which differences of opinion exist.

It is my intention to request the National Academy, The Smithsonian Institution, the Society of American Naturalists, the American Ornithologists' Union, the American Association for the Advancement of Science and the Royal Society of Canada, each to appoint one of its members as a representative upon an Advisory Committee to which I may submit for approval or disapproval all of the questions which I intend to support in the meetings of the International Commission, and with which I may advise regarding concessions to be made or requested in those points upon which American opinion differs from the views held in some of the other countries. It is needless to add that my vote in the International Commission will be determined by the action of this Advisory Committee, should the occasion arise that my personal opinion upon any particular point differs from the opinion of the gentlemen appointed to advise me.

CH. WARDELL STILES,

B. A. I., U. S. Dept. Agriculture.

WASHINGTON, D. C.

KATYDID ORCHESTRATION.

TO THE EDITOR OF SCIENCE: I am interested in the notes lately published in SCIENCE on this subject, and should be very glad to have the insect described by Mr. Gould identified, for my own information. This is because, during two summers spent at Cranberry, in the mountains of North Carolina, not far from Roan Mountain, I continually heard the same music Mr. Gould describes, and have no doubt that my insect and his are the same. Supposing it to be, of course, well known to entomologists, I preserved no specimens, and only remember being struck by the very small size of those I handled. It is certainly not the common Katydid, *Cyrtophyllum concavum*. Aside from the almost deafening noise which seemed, as it were, to make the trees tremble, what struck me most was the punctuality with which the orchestra tuned up at a particular time near sunset, the regularity with which the performance continued through the night, and its conclusion at a certain hour in the morning. The inhabitants of the place also speak of the seasonal periodicity of this insect, which is so perfect that they draw certain weather prophecies from its acceleration or retardation of two or three days.

ELLIOTT COUES.

WASHINGTON, 1726 Street.

November 2, 1895.

TO THE EDITOR OF SCIENCE: The antiphonal rhythm of two 'orchestras' of Katydids is so very familiar to me that I was rather surprised to read of it in Dr. Gould's letter of September 20, supposing, as I did, that what I regarded as such an ordinary occurrence must, of course, have been noted and explained by entomologists. But the letters of Prof. Smith and Mr. Scudder in your issue of November 1st imply that such is not the case. My testimony, therefore, may be of some interest. Dr. Gould's description fits exactly the phenomena noticed evening after evening at my home in Montclair, N. J. The 'antiphony' is often very regular for several minutes, sometimes stopping short and again becoming broken into irregular individual stridulation at the end. I have sometimes thought that the exact unison of movement

might be purely mechanical and analogous to the control of one vibrating body by another, the medium of control in this case being the sensitiveness of one insect to the stridulations of another. But the fact that they often start all at once seems to bar out this hypothesis, and, indeed, is a fact difficult to account for in any way.

As to the pitch, it certainly seems as if one 'orchestra' were from a semitone to a tone removed from the other, but, as Mr. Scudder suggests, this may be only apparent. In case it is real, however, may it not be due to the falling into beat of each insect with those to whose stridulation it is most sensitive—namely, those that produce sounds approximating to its own in pitch?

ARTHUR P. BOSTWICK.

NEW YORK, November 5th.

THE SCIENCE OF MENTATION.

EDITOR OF SCIENCE: Some time since a reference was made in SCIENCE to a paper published with the above title in the *Monist* for July. The author was reported to have studied by experimental methods the development of certain forms of 'mentation' in dogs. As I have been greatly interested in the subject of comparative psychology for years, and have myself been devoting much time to the study of the psychic development of animals from birth onward with investigation of the contemporaneous changes of a physical kind especially in the brain, I looked up the article referred to, written by Mr. Elmer Gates. Many of the statements and conclusions are of so remarkable a character that I should be glad to get further information, as would, no doubt, others also. We are told that seven shepherd puppies were confined in a completely darkened room for nine months; that the mother was permitted to go in and out; but we are not informed as to whether the mother was admitted for the sole purpose of suckling the puppies, though this is the natural inference. Now, if a dam is capable of supplying seven puppies at nine months of age with all the nourishment they require, as one specially interested in dogs and who has for years made a special study of these animals and bred them extensively, I should like to know the facts; for nothing of like kind is, so far as I am

aware, on record, and on the face of it I should doubt the possibility of such a thing. I see no necessity for any such drain on the dam, yet Mr. Gates' paper leaves the matter in doubt.

Again, though the most sweeping conclusions are drawn as to results both positive and negative following functional use and disuse, of certain portions of the organism, and though these experiments stand almost or quite alone, but meagre details are given either of the experiments or the anatomical appearances, and not a single illustration either diagrammatic or other accompanies the paper, nor is there any intimation that such details or illustrations have been or are to be published elsewhere. I should like to point out that such work is of but little use to scientific men in its present form, for at best it is only suggestive, not demonstrative. It is to be hoped that if Mr. Elmer Gates can furnish the details and illustrations necessary to meet scientific requirements he will lose no time in doing so, as, if his experiments, etc., are reliable and his conclusions correct, they are not only of great scientific interest but of much practical importance to educationists and others. Mr. Gates' paper abounds in very stimulating 'mentation,' and much of it seems to fit very naturally into my own mental moulds. In asking for more details I think that I am writing in the interests of a large class of scientists and others.

WESLEY MILLS.

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INVERTED IMAGE ONCE MORE.

IF Prof. Woodworth (see SCIENCE, October 25, p. 555) will look into my little volume on *Sight*, pp. 87 and 88, he will find described and explained not only the phenomena he refers to, but all his experiments with the lids. I have been familiar with the phenomena all my life, but first described and explained it in 1871 (see *Phil. Mag.*, Vol. LXI., p. 266, 1871). I afterwards discovered that it had been previously explained by Priestley. It is not due to imperfect accommodation, as Prof. Cattell thinks, but to *refraction by the concave watery meniscus between the two lids and the surface of the cornea*. The following figure will explain itself and the phenomena in question. The central ray c ¹